
NORTHUMBERLAND & DURHAM
MEDICAL SOCIETY.

NOVEMBER 13, 1884.

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NORTHUMBERLAND AND DURHAM MEDICAL SOCIETY.

THE SECOND MONTHLY MEETING was held in the Library of the Newcastle-on-Tyne Infirmary, on Thursday, November 13th, 1884—the President (Dr. Fielden) in the chair.

The following gentlemen were elected members of the Society:—

John Whitehouse, M.R.C.S., Park Road, Newcastle.

W. G. Ridley, M.B. Durh., M.R.C.S., Infirmary, Newcastle.

The following gentlemen were proposed for election:—

J. L. Crisp, L.R.C.P. Edin., M.R.C.S. Eng., South Shields.

Herbert Bramwell, M.B. C.M. Edin., Ingham Infirmary, South Shields.

James Campbell, M.B. C.M. Edin., Blackhall Mill.

M. A. Savage, M.B. C.M. Edin., Chilian Service (Consett).

On the motion of Dr. ARNISON, it was unanimously decided to hold the December meeting on Thursday, the 4th, instead of Thursday, the 11th of the month.

DISCUSSION ON THE PREVALENT DISEASES OF THE DISTRICT.

Mr. HENRY E. ARMSTRONG presented the following:—

Return of Admissions to the Newcastle Fever and Small-pox Hospitals during the month of October, 1884.

Disease.	Admissions.	Deaths.
Scarlet Fever.....	10	0
Enteric Fever.....	4	0
Small-pox.....	2	0
Total.....	16	0

Mr. HENRY E. ARMSTRONG said he had no remark of moment to make in reference to scarlet fever, except in regard to a case he had to do with some seven weeks ago. A boy was removed from a house suffering from scarlet fever. The case presented nothing peculiar; but about a week ago, six weeks after his removal to the hospital, another case occurred in the family, and, with the strictest investigation, he could not find any cause to account for it. Had the first case been dismissed from the hospital, he would naturally have thought the patient had taken infection home with him. There was, however, no connection between the cases, and the

second child had not been out of the house to catch infection. The second case was also removed to the hospital, and while both children were in the hospital, two or three days ago, two further cases of malignant scarlet fever occurred in the same house. It was a self-contained house, all the family lived on the same floor, and there was nobody else to infect them. One of the cases died after some hours of illness. In the first case the removal was effected at once, and so far as he knew there was thorough disinfection. The second case occurred six weeks after the first, and he could not make out where infection came from. He had his attention drawn to the duration of the infection in these particular cases. Mr. Armstrong presented the Society with a copy of the report on water prepared by the City Engineer and himself.

Dr. LUKE ARMSTRONG referred at length to the frequent postponement of scarlet fever to a late period after exposure to the infection, and remarked that he scarcely thought that the Medical Officer of Health need be surprised at witnessing a second case six weeks after the occurrence of the first. Dr. Armstrong adduced an example which happened in his own practice, to prove the fact that scarlatina poison may lie for a long time dormant.

PATHOLOGICAL SPECIMENS.

AN OVARIAN TUMOUR, WEIGHING 14LBS., WITH SUPPURATING CYSTS.

Dr. NEWCOMBE said: The patient from whom I removed this cyst was 29 years, married, and had two children. She was a spare, delicate woman, and came to me twelve months ago, when I detected a tumour about the size of an orange in the left iliac region. I diagnosed it to be an ovarian tumour, and advised its removal. I did not see her again for ten months, and by that time it had grown very large, and she desired its removal. As her circumstances were not good, I sent her into the Infirmary, where, for some reason, she remained only several days, and on coming out asked me to operate. I consented, and performed the operation on October 10th with the assistance of Drs. Heath and Green. Upon cutting into the peritoneum about two gallons of ascitic fluid escaped. I found the tumour adherent to the descending colon and other parts of the bowels, and to the large omentum; and on inserting a trocar into one of the cysts, a large amount of pus escaped. I then enlarged my first incision, and drew the tumour out in its entirety, tied the pedicle with antiseptic silk, cut off a portion of the omentum which had an unhealthy look, tying the bleeding vessels with catgut, and applied the cautery to other bleeding points. On examining the right ovary, I found a suspicious

growth, having the appearance of a small cyst, so I removed it also. After carefully sponging away all fluid from the pelvis, I sewed up the wound, and put in a large glass drainage tube (Keith's), and I consider that the subsequent recovery of my patient was greatly due to this device. I dressed the wound with green protective and gauze for the first two days, and afterwards with iodoform wool. I did not use carbolic spray. The first three or four days a large amount of bloody serosity discharged itself through the tube. She made an uninterrupted recovery; her temperature never rose above 100·8. She took nothing by the stomach for the first three days but a teaspoonful of cold water occasionally, but was fed by beef tea and brandy injections, alternating with milk and white of egg, at regular intervals, which were well retained. She never had any sickness. The catheter was passed thrice every 24 hours. On the 13th some beef tea was given by the stomach and retained, and the bowels were relieved by an enema of castor oil. She never had any pain, and slept well. On the tenth day I removed the large glass tube and substituted a small india-rubber one, and, except a small discharge of pus for three or four days, which did not cause any inconvenience and soon stopped, she has done well. The wound is healed. She sits up every day, takes food well, and is gaining strength; and I consider her recovery under such adverse circumstances very gratifying.

Dr. PHILIPSON stated that the patient referred to was, he believed, admitted to the institution under his care. After carefully examining the patient he came to the conclusion that she was suffering from an abdominal tumour which was ovarian. He consulted Mr. Page, who carefully examined the patient, and they came to the conclusion that she was suffering from an ovarian tumour, and that the case was a suitable one for operation. Having got the opinion of the staff, she went to be operated on by the surgeon under whose care she was before she came to the Infirmary.

Mr. PAGE said he could quite confirm what Dr. Philipson had said. It was a favourable case for operation. The nature of the complaint was explained to her, and she told them she had come merely for the purpose of getting the opinion of the staff. She accomplished the object for which she came, and it was no slight to the institution that she went away again.

Dr. NEWCOMBE explained that it was by no means his intention to cast a slight upon the Newcastle Infirmary in referring to the circumstances of the case.

SCAPULAR TUMOUR.

Dr. MORRISON said: The scapular tumour I have here was removed from a man, 31 years of age. He had noticed its growth

during the past three months. He was a miserable, emaciated man, and had the appearance of being consumptive. His history was that he had previously enjoyed fairly good health, but had been very intemperate until about two years before. He was admitted to the hospital under my care. On examination of the internal organs nothing was discovered wrong except a considerable enlargement of his liver. He said the pain was so severe that it prevented him from sleeping and eating, and that during the last three weeks he had lost a stone in weight. This determined me, in spite of his unfavourable condition for operation, to remove the tumour with this portion of the scapula. The tumour was limited to the vertebral half of the scapula; it was considered sufficient, therefore, to remove only a part of it. Consequently I determined to remove the scapula from a line drawn from the supra scapular notch to the groove on the under surface of the scapula, where the dorsalis scapular artery crosses on to the body of the scapula. I first made an incision along the vertebral border of the scapula, then across the line I have indicated, passing from the upper border of the scapula to the lower, near its acromion end; then joining the two by an incision along the spine of the scapula, I reflected two square flaps upwards and downwards, and divided the muscles attached to the vertebral edge. Next, dividing the teres major, I passed my finger deeply under the remaining structures connected with the lower border of the scapula in the direction of the axilla, and following the finger up with one blade of this pair of forceps (Sir Spencer Wells' omentum forceps), grasped all the tissues, including the subscapular artery. After dividing the muscles attached to the scapular spine, I secured in the same way the supra scapular artery. Then, dividing the muscles across the scapula in the supra and infra spinous fossæ, I sawed through the scapula at the point, as you will see by examining the specimen, which I have previously described. The scapula and tumour were now easily removed by dividing the remaining muscular attachments underneath. The whole operation was done antiseptically, and the wound dressed in the usual way. The man lost little blood at the time of the operation, but seemed to suffer much from shock. By evening he had recovered, and was satisfactory when the house surgeon saw him late at night. Early next morning he seems to have taken a sudden change and died. Unfortunately, as I could not get a *post-mortem*, I cannot account for the death.

OVARIES REMOVED FOR PELVIC PAIN AND MENTAL SYMPTOMS.

The second specimen is one of two ovaries, the other of which has been reserved for pathological observation. The patient was the mother of a family of four. After the birth of a child, some 18

months, she did not feel well. Eight days after confinement she felt something wrong with her pelvis, and her sensations were so disagreeable that she had to go back to bed, where she remained for about nine months, when she was admitted to the Hartlepool Hospital, under my care. On examining her I found a retroflexion of the uterus. I gave her chloroform and reduced the retroflexion to make a complete examination, but she went out no better than she came. After a time she developed mental symptoms, became obscene in her language, and so disgusted her friends that no one would go near her. She fancied her husband was guilty of immoral practices, and she still complained of the disagreeable sensations she had at the commencement. She was then sent to the workhouse with a view to transference to a lunatic asylum; but, after being there a week, she pressed her husband to take her home again out of the workhouse—which he did, when she became more troublesome, if possible, than before. She was again admitted to the hospital complaining of pelvic pain, bearing down, and so much pelvic uneasiness when she attempted to walk as to prevent her continuing her efforts. In face of the fact that her pains and mental disturbance had been worse during the menstrual period, and had commenced after her confinement, and nothing uterine could account for it, I thought that by removing her ovaries matters might be improved. The operation was accordingly done. I found the right ovary in its normal position and free; the left was adherent to the pelvic brim, and occasioned a little difficulty in the operation. The uterus was found to be retroflexed, and was stitched to the anterior abdominal wall, where it remains firmly fixed. Her immediate recovery from the operation was unaccompanied by any rise in temperature or other symptom. Now, eight months after operation, she can perform her household duties and has no pelvic trouble, but is still a complaining woman, though very much improved. I am not prepared to say that the severe operation, the moral effect of preparation and after-treatment, have had no effect; but the fact remains that after losing her ovaries she has steadily improved. As you will see from observing the specimen, there are several small cysts at different parts of the ovary, which point distinctly to some pathological change.

OVARIES REMOVED FOR MYXŒDEMA.

The next specimen I have to show is a pair of ovaries removed from a patient 28 years of age, suffering from myxœdema. Her history is that, after a severe mental trial, a year ago, she began to have menorrhagia. This gradually increased until, finally, she had never a fortnight free from profuse loss. Anæmia followed, and along with that general swelling of the whole body. Being in doubt as to whether the swelling was simply œdema, or

whether the symptoms were due to myxœdema, I sent her to Dr. Drummond, who decided that she had myxœdema, a diagnosis which the sequel has proved correct. After having free monthly hæmorrhages she was always decidedly worse; and as nothing we gave her or did availed to check it, with Dr. Drummond's approval, I removed her ovaries. Her recovery from operation was uneventful. For six weeks after the operation there was no bleeding, but at that time there was a period of hæmorrhage as profuse and lasting as before. Since then there has been no discharge of any sort, and her general health has correspondingly improved. There was nothing pathological found in either uterus or ovaries. The myxœdematous conditions remains in *statu quo*.

Dr. LUKE ARMSTRONG showed a glandular tumour removed from behind the jaw of a boy.

TWO TUMOURS CONNECTED WITH THE POPLITEAL NERVE.

Dr. HUME said: A month ago I received under my care in this Infirmary a boy suffering from a tumour of the popliteal space, about the size of a goose egg and very hard in texture, freely movable from side to side, with the feeling that it was fixed from above downwards. This tumour had grown in the space of three weeks. It was lying over the line of the vessels in the popliteal space, and from the character of its mobility and rapidity of growth it was diagnosed as a sarcoma connected with the popliteal nerve. Having obtained the parent's permission to amputate, if necessary, I cut down upon the tumour and found the diagnosis to be confirmed. An attempt was made to dissect out the growth, but it was found to be intimately connected with the internal popliteal nerve, and with the ligaments of the joint. The limb was therefore amputated. I have placed under the microscope a section of this tumour, and in some parts of the tumour the cells are so large as to justify the name of myeloid sarcoma being applied to it. Although it was so intimately connected with the popliteal nerve, the boy had no pain shooting down his leg, and had no difficulty in walking. Under my care there was at the same time a girl who had also a tumour in the popliteal space, which I at first thought was a bursal tumour of the semi-membranosus tendon. When the patient was placed under chloroform, and a more satisfactory examination was made, it became evident that the tumour was solid, and was not quite in the position of one of the ordinary bursal tumours. I therefore cut down and found it to be a tumour of the size of a walnut, attached to the internal popliteal nerve. There was some difficulty in getting it free from the nerve at one part, and I had to divide a branch of considerable size which passed through the lower part of the tumour. On examining the tumour

it was found to be glandular. I have placed under the microscope a section which shows the ordinary lymphatic gland structure. It is interesting to note that in this case, unlike what occurred in the more serious case of the boy, the patient complained of great pain shooting down the leg, and of a cramping character, which greatly impeded her walking.

INTRODUCTION TO A DISCUSSION ON "THE ETIOLOGY OF PULMONARY PHTHISIS."

By GEORGE HARE PHILIPSON, M.A., M.D., D.C.L., F.R.C.P., Senior Physician to the Newcastle-upon-Tyne Infirmary, Professor of Medicine to the University of Durham, &c.

MR. PRESIDENT AND GENTLEMEN,

The responsibility of introducing the important subject, "The Etiology of Pulmonary Phthisis," for discussion, has been committed to me, an honour which I duly estimate, a selection for which I beg to express my grateful appreciation.

The causation of pulmonary phthisis is of universal interest, widespread in its bearings, and in importance unequalled in medicine. In the light of recent discoveries, in active speculation of their true import, previous propositions may have to be reconsidered. The action of the committee in selecting this subject is deserving, therefore, of our hearty acknowledgments; for it may be fairly anticipated that valuable and important facts will be elicited in support of or in contravention of the views that may be enunciated.

In the way of introduction it will be proper to define the condition at issue. The disease is characterized by progressive wasting of the body, persistent cough, expectoration of muco-purulent material, sometimes of blood, loss of strength, shortness of breath, hectic fever, night sweats, and diarrhœa.

From a pathological point of view, pulmonary phthisis strictly includes the morbid processes of the lungs, characterized by the formation of consolidations in a granular or a diffuse form, which either undergo caseation and disintegration, leaving behind excavations in the lung tissue, or becoming indurated and shrinking, cause contraction of the affected organ.

Tubercle is not the only morbid product which induces these results. The consumptive process may originate from lung inflammation, new growths, or vascular conditions.

Phthisis may result, directly, from an attack of acute croupous pneumonia. Catarrhal pneumonia, either acute or chronic, is the variety of pulmonary inflammation to which the origin of a large number of cases of phthisis may be attributed.

The usual new growth which originates phthisis is tubercle. The breaking down of syphilitic gummata is another new growth which will originate phthisis.

Occulsion of branches of the pulmonary artery probably contributes to the destructive process in some cases of phthisis. Embolism of the pulmonary branches, and consequent localized gangrene of the lung, is another vascular condition. Intensely

congested and œdematus lung tissue may break down and form cavities.

It is highly probable that phthisis may originate, in different cases, in either of the ways indicated. Unquestionably a large number of phthisical cases are attributable to causes acting more or less directly upon the pulmonary organs, which tend to excite some form of inflammation, and catarrhal pneumonia certainly does seem to be frequently the primary morbid condition which leads to the destruction of the pulmonary tissue. It must, however, be borne in mind that tubercle may probably result from local irritation or infection, and it may be thus produced along with pneumonia. Moreover, one condition may soon set up another, and so the destructive process may be a complex nature from the outset, or from a very early period.

This twofold arrangement into general and local causes is clearly indicated. In the general, the constitution of the individual, and the functions of nutrition and assimilation would appear to be first involved. In the local, the lungs are the primary seat of the disease, and changes in their epithelium and parenchyma lead to products of a retrograde kind, through which the lymphatic and vascular systems, and the constitution generally, become infected. General causes affect the whole system, such, for example, as family predisposition, climatic influences, dampness of soil, and infection. Amongst local causes are to be enumerated inflammatory affections of the lungs and pleura, trades and occupations giving rise to a gritty atmosphere, and injuries to the chest.

Having indicated in a general manner the etiology of pulmonary phthisis, through its pathological relations, the consideration of the special questions, viz., family predisposition and infection, as causes of the condition, will be entered upon.

At the present time the most diverse opinions are held in reference to the question of infection. On the one hand, we have eminent pathologists holding strongly to the view that pulmonary phthisis, dependent upon tubercle, is infectious, and they are supported by the belief deeply impressed upon the popular mind in Italy and other hot countries, that the disease may be conveyed as readily as ordinary zymotic complaints, not only from person to person, but even by means of bedding and clothes.

On the other hand, we have the testimony of physicians to hospitals for consumption entirely opposed to this view, and, notwithstanding the fact that about one-half of the mortality in this country between the ages of twenty-five and thirty-five is due to pulmonary consumption, there are thousands of English medical practitioners who have never seen a case of undoubted tubercular infection.

The popular and professional attention to the question has been incited by Koch's discovery of the bacillus of tubercle.

Quite recently, also, inquiries on the subject were addressed by the Collective Investigation Committee of the British Medical Association to every member of the British Medical Association.

That, in a certain sense, tubercle is an infective disease, cannot be doubted. Long before the discovery of the specific bacillus, it was proved by Villemin and others that tuberculous material would convey the disease to sound animals, when it was introduced into their bodies by puncture or inoculation. Animals fed with it contracted the complaint, and the inhalation of dried and pulverised sputum from consumptive patients caused the disease in dogs and other animals.

Moreover, as Cohnheim has shown, the course taken by the complaint in the animal body, as in the human subject, completely justifies the denomination of infective which was applied to it by Burdon Sanderson; and this remark holds good however the disease may have been contracted, whether arising naturally or introduced by inoculation.

Tuberculous material always follows the track of the absorbents of the part infected. Implanted in the peritoneum, it produces—first, tubercular peritonitis, and then tuberculosis of the spleen and liver. Injected into the anterior chamber of the eye, it attacks the iris. When the disease has been produced artificially by feeding animals with tuberculous substances, it is first observed in the glands of the bowels and of the mesentery. After inhalation of dried and pulverised sputum, it appears in the lungs and bronchial glands. And, lastly, when subcutaneously injected, it makes its way to the nearest lymphatic glands.

It seems, indeed, highly probable that acute tuberculosis in man or animals arises only when the peculiar virus obtains an entrance into the general circulation.

And we cannot ignore the significance of the facts that have been placed before us by Koch. He has proved, beyond doubt, that tuberculosis is due to an organism, one of the large group of desmo-bacteria. It has never yet been found in any other than tubercular disease, and its presence is so constant, not only in tubercular organs, but also in the sputum from phthisical patients, that search for it has now become, in doubtful cases, a necessary branch of clinical work.

By a series of cultivations in sterilized ox blood serum this bacillus has, moreover, been obtained distinct and separate from other organisms, and has been proved to possess the power of generating tubercle in animals into whose bodies it has been introduced by inoculation, injection, or inhalation.

But it may well be objected that, although tubercle may be due to a parasitic growth, and that it may thus be infective within the body in the sense that after inoculation it spreads infectively

through the body, yet this is no answer to the questions whether or not tubercle is capable of being conveyed by ordinary methods of contact from person to person, nor as to the conditions that are necessary to its propagation. It will be needful, then, to see—first, what proofs are forthcoming that the germ is implanted from without, and under what circumstances it is so conveyed; and second, to what extent direct contagion between individuals has been observed.

As to the transmission of tubercle through media external to the body, Cohnheim enunciated that a tuberculous or scrofulous product is mostly deposited in those parts of the body that are either most exposed to attack from without, or in which any virus coming from the outside may lodge for the longest time.* Thus no internal organs are so constantly brought into relation with the atmosphere as the lungs, and accordingly no other organs are attacked by tubercle with the same frequency and the same intensity. In many cases of this disease also, all the other organs not connected with the lungs are free from its attack.

This fact cannot well be accounted for on any other supposition than that of a primary and immediate attack upon the respiratory tract by the virus. Next in order to the lungs and pleuræ, and to the bronchial and tracheal glands, come the glands about the pharynx; and the next after these, owing probably to the swallowing of sputa, we find the lymphatic apparatus of the wall of the intestine, the isolated and agminated follicles, most liable to be the seat of tuberculous ulceration. The œsophagus escapes contagion because of the rapidity of the transit of the virus; and the stomach, perhaps, owing to the presence of the gastric juice. Through the intestinal ulcers a path is opened to the bacillus into the mesenteric glands, to the liver, and to the whole of the rest of the body, thereby producing general tuberculosis.

In fact, long before the discovery of the specific bacillus it had been shown that tubercle is intimately associated with the lymphatic system.

Rindfleisch declared that “specific irritation of the endothelia of the lymphatics, the serous membranes, and the blood vessels is the essential factor in the production of the miliary nodule; and it is only because the lymphatics run by preference in the immediate neighbourhood, in the adventitia of the blood vessels, that miliary tubercles exhibit a preference for that locality.”†

Wagner referred to the extension of tubercle in the course of lymphatic vessels, or in that of blood vessels.‡

* Cohnheim, *die Tuberkulose von Standpunkt der Infectionslehre*, p. 20.

† Rindfleisch, *Pathological Histology*, vol. i., p. 141.

‡ Wagner, p. 449.

In the points of attack chosen by the organism, and in the course which it takes throughout the body, we see then clear proof—first, that the poison is derived from without ; and second, that although in rare cases it may be taken up by the intestinal tract, its usual point of entrance is by the air passages ; and we might hence conclude, without further evidence, that its favourite vehicle would be found in the atmosphere. Direct evidence on this point must, however, be brought before you, and is most difficult to find. But before doing this, let me clear the ground of other material, and allow me to pass in review the several other conditions that have been found to favour the production of tubercular disease.

Let us, then, try to discover which of the so-called causes of tubercle are only predisposing or favouring influences, and which are necessary to its genesis and propagation. We shall probably thus throw some further light upon the question as to its truly infective character.

First amongst such causes must certainly be placed hereditary tendency to the disease ; but how seldom are the germs actually transmitted from parents to offspring, and how constantly is the disease acquired in after life. Moreover, a large proportion of cases of consumption arise in families that have not been known to have lost any member from this cause within any recent period, or within the last generation. Welch, in his prize essay, “On the Nature and Varieties of Destructive Lung Disease,” recounts that sixty per cent. of the cases of phthisis amongst soldiers are non-hereditary, and owing to the medical supervision of recruits it may be fairly assumed that most of these cases arose after the commencement of their military service.

Even if we grant that statistics on this point are defective and unreliable, heredity cannot be regarded as an essential condition of tuberculosis. We may say, on the contrary, with Louis, that “very few people are born necessarily to die of consumption.”

As Villemin has said, “at best the influence of heridity is an affair of the transmission of an aptitude to contract the disease.”* Though we may hesitate to follow him when he further declares that “it has nothing to do with physical conformation, constitution, or temperament,” and that “there is no such thing as a tubercular diathesis.”†

Viewed from the standpoint of present knowledge, we may say that, except in the case of infants born tuberculous, it is in the highest degree improbable that the tubercle bacillus could produce spores that would rest so long as they must be supposed to do, on the hypothesis of the hereditary transmission of the disease.

* Villemin *Etudes sur la Tuberculose*, p. 289.

† P. 299.

The term "family predisposition" is substituted for "hereditary predisposition," because the latter, from its limitation to direct descent, necessitates the omission of the evidence of disease in collateral relatives. The statistics of the first Brompton Hospital Report on this point, as compiled by Quain, who contrasted them at the same time with the statistics of insanity, and those of Cotton and Fuller, show that among the lower classes hereditary predisposition—that is, where one or both parents were affected, was traced in 25 per cent. Theodore Williams' researches among 1,000 cases of the upper classes show 12 per cent. of direct hereditary predisposition, and 48 per cent. of family predisposition.

Family predisposition is more common among women than men, in the proportion of 57 to 43, which may be accounted for by the more sedentary and less invigorating life of the former. The transmission of phthisis is more common through the mother than through the father; but where one parent alone is affected, fathers transmit more readily to sons, and mothers to daughters, than the converse. Pollock lays stress on the influence of hereditary predisposition in the acute forms of phthisis, and states that out of 179 cases, only 34 could positively declare absence of family taint. The principal effect, however, of family predisposition is to be seen, not in any peculiarity of symptoms, but by the influence it exercises over the age of the attack. It is much earlier in patients so predisposed than in others; and in females this influence is greater than in males.

Climate has often been accused of causing consumption, and especially the changeable humid climate of this country, but we find no support to this doctrine from the geographical distribution of the disease.

It is not surprising that, at one time, tubercular disease should have been ascribed to some form or another of want of proper nutrition, and especially to deficiency of fatty food. No one can doubt that want of sufficient food and mental distress are amongst the most powerful agents in preparing the ground for the successful sowing of the seeds of consumption, but even if we had not known of the existence of a specific organism, and indeed before it was known, it had been already long apparent that these causes are predisposing only and not essential.

Hardship, exposure to the weather, cold, and certain occupations, especially those carried on in-doors, or accompanied by dusts of various kinds, have also been ranked amongst the causes of phthisis. But we may at once put these aside as in no way essential causes, though they may prepare the way for the reception of the specific virus.

There yet remains to be mentioned the one antecedent of consumption, without which none of the other predisposing influences

that we have mentioned, however potent they may be, can do their deadly work. This cause is foul air, or, to limit the scope of this phrase more strictly, it is air rendered impure by respiration.

Before the discovery of the specific element of tubercle, Simon expressed his opinion that "the common (septic) ferment, which in its stronger actions quickly destroys life by septicæmia, can, in slighter actions, start in the infected body chronic processes which will eventuate in general tubercular disease."* At the present time, however, this simple pythogenic theory, as it might be called, is quite untenable, and McCormack showed true prescience when he brought forward his doctrine that it is to "rebreathed air" that pulmonary phthisis is to be ascribed.

The history of phthisis in the British army at home and abroad, affords, perhaps, the best proofs of the overpowering influence of bad ventilation, assisted in its work by the influence of bad drainage.

Farr, in his report to the Army Commission after the Crimean war, remarked that "the prevalence of phthisis in the armies of Europe is probably due in part to the inhalation of expectorated tubercular matter, dried, broken up with dust, and floating in the air of close barracks.†

Welch, of the Army Medical School, Netley, showed that consumption was the great chronic devastator of our army, in spite of all the selecting influence of recruiting regulations, and in spite of every variety of climate. At the time of which Farr wrote, the rate at which soldiers died from consumption was uniformly high in the most varied stations, and in the most beautiful climates of the world—in Gibraltar, Malta, Ionia, Jamaica, Trinidad, Bermuda. One condition was common in all these different places, namely, the faulty ventilation of barracks, or of ships, and the consequently vitiated atmosphere which the men had to breathe.

The Sanitary Commissioners for the Army reported in the year 1858 that the Royal Foot Guards died at the rate of 20·4 per 1,000, whilst a similar number of civilians showed less than 12 deaths per annum, and the number of deaths from lung disease in the former was 12·5 to 5·8 of the latter.

They pointed out that in civil life, insufficient clothing, insufficient and unwholesome food, sedentary and unwholesome occupations, and the vitiated atmosphere of unhealthy dwellings, all contributed to the propagation of this class of diseases. But in the army it cannot be alleged that the clothing, the food, or the nature of the occupation in itself are of a character which would justify the imputation that they are among the predisposing causes of the excessive mortality of the soldier by pulmonary disease.‡

* Simon : Supplementary Report to the Privy Council, 1874, No. ii., p. 16.

† Farr : Dictionary of Hygiene.

‡ Welch "On the Nature and Varieties of destructive Lung Disease, as seen amongst Soldiers."

What was the cause? The Commissioners did not hesitate to reply. Though certain other causes might be in operation, "the ravages committed in the ranks of the army, by pulmonary disease, are to be traced in a great degree to the vitiated atmosphere generated by over-crowding and deficient ventilation, and the absence of proper sewerage of the barracks."

The evidence from jails, workhouses, and schools, is all to the same purport, and is given in Parkes' Hygiene.

We may, then, with much confidence support McCormack in his theory that the chief agent in producing consumption is air that has been polluted by respiration; and if this conclusion is once reached, the opinion is greatly strengthened that the bacillus is derived from human lungs, and that in some mode or other it is conveyed by the air into the lungs by other people.

But we must carefully guard ourselves from deducing the further proposition that tuberculosis is infectious in the ordinary sense, and that it is directly conveyed, like an exanthematous disease, from person to person. It may well be that there is another and a necessary stage between the shedding of the germ and its fructification in the form of genuine phthisis, and thus there may be no direct transmission. It behoves us then to consider carefully what is the evidence with respect to the actual communicability of the disease, by contact or by fomites.

That contagion from a consumptive patient is a possible event is an opinion that has been held by many eminent men, Morgagni, Van Swieten, Valsalva, Morton, and Baumès amongst the ancients; Laennec, Copland, Bowditch, William Budd, Gueneau de Mussy, and Hermann Weber, in more modern times. All these observers have expressed their views that the disease is contagious, and although there are many others on the opposite side, affirmative evidence is in this case of more value than negative. Quite recently, too, a number of supposed cases of contagion have been contributed by medical men to the Collective Investigation Committee of the British Medical Association, and, on the first perusal of this record, it would seem to be impossible to doubt that, although the contingency may be a rare one, the possibility of such direct infection has been proved. A little consideration will show, however, that the importance of these cases is liable to be exaggerated, and that they afford but little certain proof of the contagiousness of phthisis.

Regarded simply as statistics, the tables of cases laid before us by the Collective Investigation Committee cannot, indeed, be said to prove anything. In the record we are happily supplied with a means of testing the results of the inquiry in a paper by Longstaff. He has calculated the "probability of the accidental and fatal incidence of phthisis upon both husband and wife" during the ten

years—1871 to 1880; and he finds that, without any infection being assumed, 4,358 cases of deaths of both husband and wife would have occurred in this period. On applying these figures to the data given in the record, it appears that about 250 of these purely accidental double deaths ought to have been noted by the 1,078 medical practitioners who have made returns. In point of fact, this number is barely reached in the tables intended to prove contagion, so that it becomes at once highly probable that many of the cases given were only accidental.

In the record, moreover, the observations extend over a variable period, dependent upon the experience of the observer; in some cases less, in some more than a period of ten years, and some extend even so far back as fifty years. In any comparison, also, it would be necessary to deduct cases occurring out of Great Britain. Again, it would be hardly right to include the 35 cases said to have only become manifest a few months after the partner's death. On the whole, as a statistical record, it appears at once to be both meagre and imperfect.

We are, however expressly told that the tables are not to be looked upon as ordinary statistics. They are affirmative only, and many cases have probably been admitted in which there seemed no distinct contagion to persons not hereditarily disposed to the disease.

We are not, then, to deal with the figures as mere statistics. Each case is intended to stand upon its own merits; and although we cannot entirely put aside the theory of probabilities, we must be prepared to admit the weight of evidence given of absence of hereditary predisposition, previous good health, and so on. Even when admitted to this more favourable bearing, however, the records afford but little proof of direct contagion. It has been suggested that some of the instances given, and notably those of the infection of several wives by one husband, were really due to the influence, in some fashion, of the syphilitic virus, and others might perhaps be referred to some form of inoculation; but a much more serious objection to the admission of the evidence as a proof of contagion is the consideration that the occurrence of phthisis in both husband and wife may simply have been due to their exposure to similar influences, and that they are thus only proofs that these influences greatly favour the production of tubercle.

But, after all, to what do these instances amount? Even admitting for the moment that contagion had taken place in some two or three hundred cases, extending over a period varying from five to fifty years, what are these amongst the hundreds of thousands of cases in which no sign of contagion has been observed; and how can a disease be regarded as ordinarily contagious upon so small a

foundation as this? A disease, moreover, that never occurs as an epidemic, but which has all the characteristics of an endemic disorder.

The absence of contagion in hospitals for consumption must also be taken into account, and from a review of all the evidence we are driven to the conclusion that whilst tubercle is a truly infectious disease in the sense that it may be and generally is taken into the body along with air rendered impure by breathing, yet that direct infection from person to person is, in temperate climates at least, one of the rarest events.

How, then, are these opposing facts to be reconciled? It is natural, in the first instance, to ascribe proclivity or immunity to the disease mainly to the influence of individual predisposition, and to say that, though the germs of the disease may be present everywhere, that human beings affected by it exist, yet that it cannot take root without a suitable soil—without either hereditary tendency to it or some other source of weakness. There can be little doubt that this influence is truly very great, but it is difficult on this theory to account for some of the facts that have been stated. It will not account, for instance, for the multitude of cases of non-hereditary phthisis amongst the magnificent troops assembled in the Knightsbridge Barracks, nor for the comparatively low rates amongst the civil population; and it in no way accounts for the difference which we have found between direct and indirect infection.

We must seek, then, for some other mode of accounting for the limitation of the virulence of tubercular infection. Can it be found in the natural history of the organism so far as it has been drawn for us by its discoverer? Koch found, first, that the bacillus would not grow except within certain accurately defined limits of temperature, namely, from about 86 deg. to 107 deg. Fah.; second, that it requires at least one week in which to develop; and third, that the best cultivation substance was a preparation of blood serum.

The second of these conditions is the one most important to our present inquiry, for it is obvious that if the organism is in an active state it will develop almost anywhere in the human body, provided it can rest in any tissue without being destroyed. The possibility, therefore, of the bacillus resting in the body, for at least one week, is the condition of most importance.

From Cheyne's observations, it appears that the epithelium of the ultimate portions of the air passages is the usual point of attack, and we may, therefore, suppose that it is necessary for the organism to effect a lodgment somewhere in this position.

But it is hardly limited to this mode of entrance into the lungs. Deposits of fine particles of coal dust—"anthracosis," as it is called

—are common in the lungs of miners and in the inhabitants of smoky towns; and other substances, such as oxide of iron, producing “siderosis,” particles of silica “chalicosis,” may also lodge in the pulmonary tissues. Klein has shown that these substances make their way into the interalveolar lymphatics by means of the prolongation of the branched cells of the alveolar septa, termed pseudo-stomata, and he accounts thus for the observation of Sikorski, who found that carmine entered freely from the cavities of the alveoli into the lymphatics.*

It can hardly be doubted that, if these inorganic particles can thus find an entrance, the bacillus of tubercle may surely do the same. The way into the body is, therefore, so to speak, open to the organism; and once planted in such soil, if it can remain there any length of time, it will find a suitable temperature and ample supplies of the nourishment that it requires. There would thus seem to be wanting none of the conditions that we have seen to be required for its development, and for the production of true tubercular disease.

There must also be, at any rate in towns, an abundance of these germs floating in the air. Consumptives for the greater part of their illness are not confined to their rooms, or even to their houses. They go about like other people, and mix in crowded assemblies of all kinds. The dust from their dried-up sputum must, then, be everywhere present, and this has been shown by Koch to be capable of producing the disease. Ransome has also discovered a bacillus undistinguishable from that of tubercle, in the vapour condensed from the breath of persons in an advanced stage of the disease.

How does it happen, then, that tubercle does not arise in healthy persons more frequently than we have seen that it does? Why does not direct infection from person to person take place even more commonly than its indirect production? What are the barriers that interpose? These are questions that are well worth considering.

In a perfectly healthy body, when every portion of the lungs is in full activity, a tuberculous infection may well be a rare event, for a specific organism has to run the gauntlet of several opposing forces. The virus runs the chance of being at once entangled in the mucous lining of the air passages, and of being speedily ejected by the action of the ciliary waves.

In some forms of lung disease, also, accompanied by copious bronchial secretion, the probability of this event is increased, especially if the accompanying cough prevents the lodgment of mucus in any of the air tubes. The immunity from phthisis of many cases of chronic bronchitis and emphysema is, perhaps, due to this cause.

* Klein on the Lymphatic System, and its relations to Tubercle, p. 82.

On the other hand, the influence of a lessened mobility of the chest, and the consequent partial stagnation of the air, is shown in the tendency of the upper lobes of the lungs to tubercular deposit. Perhaps, too, the well known tendency to tubercular infiltration of parts of the lung, consolidated by catarrhal pneumonia, may be due to the facility afforded for a prolonged lodgment of the bacillus, and the same may be said of hæmorrhagic infarction in the lungs.

It is possible that the healthy body may have the power of destroying a certain quantity of the virus by the energy of vital actions, especially by oxidation in the highly vascular organs, the lungs. The good effects of fresh air in preventing infections of this kind, as well as of every other, is admitted. It can hardly be doubted that if the air passages are all clear and well supplied with pure ozonised air, it will destroy any bacillus that may have penetrated into the recesses of the lungs.

When, however, a lung has in any part lost its elasticity, and ceases to expel its residual air from that part, and when, moreover, there is but little bronchial mucus in the air tubes, it must be very open to attack, and must combine all the conditions that we have seen to be favourable to the onset of tubercular disease. Some physicians, notably Neimeyer and Buhl, have been so struck with the fact of some local inflammation being frequently the precursor of genuine phthisis that they have asserted the almost certainty of some antecedent inflammatory condition.

These two barriers against infection, then, may account, to a large extent, for the immunity of healthy persons to the disease, but it is not so clear why they should serve to protect nurses and others in close contact with the sick and yet should permit the production of the disease in persons who simply breathe impure air. Nor will the theory of the antagonism of an abundance of fresh air to the development of the organism entirely satisfy the conditions of the problem.

It is certain that free ventilation is a great enemy, not alone to this but to many other forms of infection, and it is not necessary to assume that the organisms in these cases have been destroyed by the oxygen or ozone of the air; it would be sufficient if they were prevented from producing spores, or were attenuated in some other way.

Angus Smith has shown that even sewage water may be protected from the action of bacteria for as long as two or three weeks in summer weather, by exposing it to one hour's aeration.*

Pasteur remarks: "It is the action of the oxygen of the air which attenuates and extinguishes the virulence. To all appearance we have here what is more than an isolated fact: we must have

* Angus Smith : Rivers' Pollution Protection Act, 1876, p. 55.

reached a general principle. We may suppose that an action which is inherent in atmospheric oxygen, an agent present everywhere, has the same influence on other viruses. At any rate it is worthy of interest that possibly a general cause of attenuation exists dependent on an agent present, which is in a manner cosmical."

The whole history of the subject of ventilation as a prevention of phthisis is favourable to the hypothesis that the bacillus of tubercle is deprived of its power in the presence of fresh air, and would probably alone suffice to account for the rarity of direct infection of the healthy by the sick in general, or in special hospitals for consumption; it would also account for the diminution of the phthisis rate after the introduction of good ventilation of barracks.

But the good influence of ventilation still fails to explain the infrequency of direct contagion in the over-crowded, badly ventilated dwellings of the poor; and we have not grappled with the allegation that tubercle is more directly infectious in hot than in cold climates, an allegation that is supported by many significant facts.

To contrast a working hypothesis that will include all these points, as well as Koch's conditions, it will be necessary to assume the existence of some condition antecedent to infection by the bacillus. The organism in question, like some other infective microzymes, may take up an increase of virulence by its sojourn for a time in some medium external to the body, and that this medium is usually the impure aqueous vapour of the breath in some definite proportion in the air.

This hypothesis would seem to meet most of the requirements of the problem. It has been shown by actual experiment, as well as by the history of many epidemics, that the virus of cholera and that of enteric fever grows in intensity after its emission from the body, always provided that it meets with a suitable menstruum. Pettenkofer, indeed, bases his cholera theory on the necessity for some such occurrence, and it is well known that enteric fever is much less infectious in the immediate neighbourhood of its subjects than when the poison has escaped into drinking water, or into the sewers.

It is interesting to note, also, that just as in hospitals for consumption, so in fever hospitals medical attendants and nurses have been shown to escape contagion, and it is therefore not unnatural to surmise that the causes of this immunity may be the same in both cases.

Moreover, if the bacillus of tubercle is facilitated in its operations by its sojourn for a week or more in contact with putrefying organic matter at a constantly high temperature outside the human body, an explanation is afforded, not only of its greater infectiveness in hot climates, but also of the frequent occurrence

of phthisis in the cottage homes of the poor, where it can find quiet lurking places in which the temperature is favourable to the germ, and in which the vapours arising from a badly drained soil, as well the polluted atmosphere, will supply the natural and fitting food of the organism ; the theory may also account for not a few of the cases of apparently direct contagion, and especially for the occurrence of infected houses and infected areas.

If the doctrines now laid down are true, what an emphasis do they lay upon the sanitary teaching respecting the necessity for fresh air and good drainage. They at once place pulmonary phthisis amongst the list of preventible disorders. They free the poor consumptive patient from the odium of being a source of special danger to his attendants, and, to a great extent, do away with the dread that many people now entertain of the disease being communicable.

The PRESIDENT said he was sure they were all very much indebted to Dr. Philipson for his very interesting and exhaustive paper. He thought it was a pertinent question to enquire how far the well-known family predisposition to phthisis agreed with Koch's theory. He would like to know how early in the course of consumptive processes the micro-organism has been found. He was convinced that medical men, from time to time, met with cases where a direct contagion could be proved, and referred to a case which occurred in his own practice. A man who died of phthisis left a widow who eventually succumbed to the same disorder. An enquiry into the family history of the latter showed that she inherited no phthisical tendency on either side for a very long way back—indeed, she had a number of brothers and sisters, all of whom were alive, together with her father, mother, grandfather, and grandmother. She had been a very robust person till the time of her marriage.

The PRESIDENT invited Dr. Philipson to indicate the special points he would like discussed by the subsequent speakers.

Dr. PHILIPSON said he thought the limits prescribed by the title of his paper should be sufficient to guide the speakers. He would remind them that he had dealt specially with family predisposition and infection.

Dr. GIBSON said he thought no doubt could be entertained of the fact that a predisposition to this disease could be inherited (whole families being swept away by it), and that the disease could also be acquired absolutely *de novo*. As an instance, among many others, of the acquired disease, he thought the leader of the discussion had hardly given the importance which it deserved to inflammatory disease in the lung. Probably bronchial catarrh

never of itself gave development to tubercular disease in the lung, but bronchial catarrh was readily transmuted into catarrhal pneumonia; and although the tendency of pneumonia was mostly in a downward direction, still from time to time the upper portion of the lung tissue became involved; and the morbid bias hereby given to inflammatory deposits was very distinct. These deposits were removed with great difficulty in this region especially, and it appears to be generally recognised that their caseous remains are susceptible of change into tubercle, and thence capable of effecting changes in the lung tissue which eventuate in its destruction to a greater or less extent, constituting phthisical disorganization. There is probably no anatomical criterion of tubercle, but this difficulty becomes greatly narrowed, even in connection with the development of phthisis, when the dogma of Klein is accepted, viz., that there is no tubercle without bacillus, and no bacillus without tubercle. The bacillus, moreover, surely lives at the expense of the pulmonary tissues in the midst of which it is placed. Then the predisposition to phthisis is doubtless inherited. Not that tubercle in the parent can be directly conveyed to the child by the ovum, but a diathesis can be conveyed from parent to offspring as surely as peculiarity of feature, and the like. This developmental law is well illustrated by scrofula as a diathesis. In the excellent paper, which we have to-night the privilege of discussing, no mention is made of the influence of scrofula as an etiological agent in the production of phthisis; and yet this influence is very distinct and very extensive. There is, in fact, a direct relationship between scrofula and tuberculosis, as there is between the latter and phthisis. Anything which favours scrofula favours phthisis—crowded dwellings, bad air, bad food, depressing influences to mind and body; but the disease is never phthisis until it is tuberculosis, and probably, almost certainly, it is never tuberculosis until its diseased products are invaded by bacillus. The influence of scrofula, then, as an etiological agent is of very great importance.

Dr. DIXON said: The paper which has just been read by Dr. Philipson embraces such a wide field of enquiry that it would be difficult to limit the number of topics which I should like to notice to the time now at my disposal. The paper is exhaustive and the subject admirably treated. In what I have to say, considering the time available, the best course to take will be to confine myself to a simple expression of opinion on the etiology of tubercular phthisis of the lungs. A belief in the infectious nature of consumption has long been popular, and the most of the pathological facts of this disease have long been known; but a satisfactory explanation of the phenomena has hitherto been wanting until Koch made his discovery of the tubercle bacillus and demonstrated its life-

history. The history of this discovery is charming, and regarding the light which it has shed on the pathology of this disease, the discovery almost seems magical. I believe we must, at present, look upon Koch's theory of consumption as being founded upon observed facts. Wherever the bacillus is, in the body, there also is tubercle; wherever tubercle is, there also is the bacillus; never either without the other. The communicability of tubercular pulmonary phthisis seems to depend upon the introduction into the lungs of this micro-organism, or its germ. For the development of this disease two conditions seem to be essential—1st, the presence of the bacillus; 2nd, the existence of a predisposing peculiar antecedent state of health. It is this antecedent state which constitutes what is called "hereditary tendency," being, in fact, a state of degenerate health dependent upon the existence of certain pathological conditions favourable to the growth and development of the bacillus. These diseased conditions have been indicated in the paper, but want of time prevents me from further alluding to them. It appears to me that Koch has given us good reasons for accepting this theory of tubercular disease: but we should remember that the truth of to-day may be the error of to-morrow, and that our successors may prove that Koch was mistaken, and that something else is true.

Dr. ANDERSON said: I would like to know whether Dr. Philipson considers the presence of the bacillus at the region of the deposit, absolutely necessary to the production of the tubercular condition. As for instance, we have a patient with undoubted evidence of phthisis pulmonalis, and you will find that the patient does not die from the pulmonary disease, but probably from what we know as tubercular meningitis. We have here undoubtedly the *materies morbi* in operation in the meninges of the brain and causing death. Many such cases must arise in the minds of members present of such an occurrence. I ask if it is absolutely necessary that the bacillus should be present in the meninges of the brain in order to the deposit of tubercles in that part of the body, whereby tubercular meningitis is set up. I, to-day, happened casually to be telling the story of an instance in which I, in my juvenile enthusiasm, felt sure that I had discovered a convincing proof of the correctness of the doctrine, "Omnis cellula e cellula." When assistant with Dr. Grainger Stewart in the pathological department of Edinburgh Infirmary, and examining the arachnoid membrane from a case of tubercular meningitis under the microscope, I found a beautiful specimen of the deposition of tubercle in the cells of the membrane which had set up the inflammatory mischief and caused death. I showed the specimen to the late Dr. Hughes Bennett (the great opponent of the cell theory), who was then in the Infirmary, and asked him if he did not see several mother cells con-

taining several tubercular corpuscles, but he refused to acknowledge it, although it was confirmed by such pathologists as Drs. Stewart, Haldane, and Sanders. One thing, however, we all failed to see was the presence of the bacillus.

Dr. OLIVER said: I can only express my admiration of the manner in which Dr. Philipson has treated this subject. I think the committee and the Society are to be congratulated on the success which has followed these discussions. I am glad the committee have limited this discussion to the etiology of phthisis. We are asked to make special reference to family predisposition and infection. These are in my opinion the most frequent causes of phthisis. But the question is this, can one of these causes act without the other? Does infection alone explain most or even many cases of phthisis? I do not think so. It is said that phthisis follows in the wake of civilization, and that it is influenced more by altitude than climate. We find it more in large towns than amongst those who live an open life in the country. We find it most amongst the poor and ill-fed, amongst those, too, who are living in houses which are ill-ventilated. Phthisis is not a morbid entity ruled by the life of a specific germ. We have several forms of phthisis to deal with. Stone-masons' phthisis, for example, is not caused by a specific germ in the first instance, but is due to irritating particles entering the lung. I do not say that toward the end a bacillus is not found. Nearly all are agreed as to the existence of a special bacillus in tubercle. It is remarkable that family predisposition failed to be recognised by the later German schools as a large factor in phthisis. It is, I believe, the most important element in the causation of phthisis. I am willing to admit the existence of a bacillus and a part it plays in tubercle. I will leave much, however, that I intended to have said, and will simply say a few words upon tubercle and its bacillus. I am a strong believer in family predisposition, as I have said. On looking over my private notes, of 28 cases, I find that 10 were the offspring of phthisical parents, and that all of them died at an earlier age than the parents. That is the interesting point—the anticipation of the disease at an earlier age than the parent. My idea of phthisis, then, is that it is in great part the effect of inheritance, and is not due to a specific germ in the earliest stages. In *inheritance* there is the transmission of something—it may be a peculiar liability of the lining membrane of the lung; very often it is a flattening or narrowing of the chest, or some deformity which interferes with the expulsion of mucus from the lungs. We must all admit that people who are the subjects of distinct flattening of the chest very frequently die of phthisis; but not all who die of phthisis have misshapen chests. The view I have of phthisis is very much that which is expressed by

many others—that it is not in the first instance a tubercular process. When people whose health is undermined are exposed to wet or cold, their cutaneous vessels dilated at the time from being in an overheated atmosphere, are suddenly contracted, blood rushes to the lungs, and effusion takes place into the air cells, and so a pulmonary catarrh is started. The patient with such is too frequently exposed day after day to the ever-varying temperature of this country, and is breathing in, now and again, air laden with certain poisons, that the catarrh becomes a bronchopneumonia, and this becomes in the end a tubercular process. I regard phthisis of the lung very much in the same way as I would regard a patient in the wards of an hospital, with an ordinary external wound. So long as the surrounding atmosphere is pure and healthy, the wound heals; but let the air once become contaminated by the presence of germs, then comes constitutional disturbance and the wound becomes unhealthy. May it not be the same thing in the lungs? First a low form of inflammation in a subject who is below par—a lesion, inflammatory but not tubercular. Expose a person with this to the influence of air containing the bacilli of tubercle, and you graft, as it were, upon a simple lesion, the true character of tubercular disease, viz., infectiveness and malignancy.

Dr. DRUMMOND expressed the opinion that consumption could not be said to originate solely either in inherited predisposition, bad sanitary surroundings, or contagion; he believed that the etiology of true tuberculosis lay more or less in all three. He would, however, give enormous prominence to heredity, for he thought that in the great bulk of cases a careful enquiry would be found to elicit a family history of consumption, more or less distantly removed. It was not enough in attempting to upset this view, to assert that many cases were met with in which no history of parental phthisis could be discovered, for, as in nervous diseases, we not unfrequently found one generation skipped, when healthy parents formed a connecting link between consumptive grandparents and their own offspring. He thought one of the most striking facts bearing on the heredity of phthisis was to be found in a statement made by Hirsch, on the authority of Hörlin, to the effect that in the Island of Marstrand, in the south of Sweden, an island remarkably free from consumption, only one person had died of the disease in seven years; and at the time he wrote there were only five cases, four of which were the daughters of a mother who had died of consumption. But it was unnecessary to advance evidence in support of the great importance of heredity, and, as pointed out by Dr. Philipson in his able address, the greatest interest was centered round the question—Is consumption actually contagious? This was a question which so high an authority as

Sir Thomas Watson answered emphatically in the negative, but since his time experimental pathology had done much to subvert such a view, by furnishing us with an infective organism, as the essential pathological feature of the disease, so that we could not any longer afford to pass over the question lightly. The whole subject was fraught with the greatest difficulty, and this difficulty appeared to him (the speaker) to be increased by the fact that most observers committed the error of confounding contagion of a disease like phthisis with that of the ordinary contagious fevers. Some would assert that the difference was one of kind and not of degree, and that the sphere of contagion, as the word was ordinarily employed, must be enlarged before it could embrace consumption; but he did not think so, for it appeared to him that the difference between the two was entirely one of degree. In his opinion the affection was contagious under certain conditions, the chief of which was a predisposition to phthisis, inherited or acquired by improper feeding, exposure to foul air, or anything else that was calculated to lower the vital forces of the body. The healthy individual, on the other hand, could resist the contagion just as a person who had suffered from scarlet fever enjoyed an immunity from future attacks. It was a remarkable fact, and one to which all could testify, that after the phthisical process had commenced in the lung a habit of body could be induced, either by treatment—the administration of cod liver for example—or through physiological or even pathological processes, which was capable of staying the further spread of disease. By way of illustration he instanced the way in which tuberculosis often ceased to progress during pregnancy; or again, how the advent of amyloid disease of the various organs prone to be attacked appeared, for a time at least, to interfere with the advance of the pre-existing lung disease.

Dr. PHILIPSON thanked the members for the flattering manner in which they had received his paper, and expressed his sincere acknowledgments for the laudatory remarks of many kind friends. He said, in replying, his task would be easy, as it appeared to him that there was almost a general agreement on the matters referred to. However, it would be necessary for him to make a few remarks in answer to the questions raised. The President had inquired as to the presence of the tubercle bacillus in the early stages of phthisis. He believed that if the microscopical examination of the sputa was prosecuted with care, the bacillus would be discovered. He regarded the recognition as of the utmost importance, for if the bacillus was discovered, remedial measures could be instituted with the object of counteracting the further development of the condition. His friend, Dr. Nicholson, of Hull, had directed the attention of the profession to the value of the discovery of Koch, in distinguishing cases of catarrhal pneumonia from incip-

ient tubercular phthisis, and he could endorse this opinion. When the physical signs were unpronounced, the value of the finding of the bacillus was greatly enhanced. To the point raised by Dr. Gibson as to inflammatory affections of the respiratory organs leading to the development of phthisis, he expressed his opinion that in these cases, circumscribed consolidations had occurred which had undergone caseous degeneration ; but they were not examples of true tubercle, and that when true tubercle existed, the bacillus of tubercle would be found. As to the intimate relation of scrofula to tubercle, referred to by Dr. Gibson, he said that scrofula was a distinctive constitutional condition, generally inherited from one parent or from both, the chief manifestations of the condition being glandular enlargement, which swellings primarily were of the nature of hyperplasia, but which might become tubercular. The third important question referred to by Dr. Gibson was respecting Professor Burdon Sanderson's experiments as to the inoculation of dogs with tubercle, when caseation was produced, not true tubercle. He was of the opinion that as a result of the inoculation inflammatory exudation followed, which in the process of degeneration became caseous. He imagined that there must have been some special determining cause in Professor Sanderson's cases, as Villemin proved long before the discovery of the specific bacillus that tuberculous material would convey the disease to sound animals, when it was introduced into the bodies by puncture or inoculation. The case referred to by Dr. Anderson he regarded as a case of general tuberculosis, and not of tubercle of the lungs, but of that form designated acute typhoid tuberculosis. He was very pleased that his colleague, Dr. Oliver, was in agreement with him, more especially as to the important points respecting hereditary influences, and also respecting the presence of the bacillus in the early stage of pulmonary phthisis dependent upon tubercle, and of the value of the recognition of the bacillus in the diagnosis and in the determination of the future of such a case. With respect to the remarks of his colleague, the distinguished Honorary Secretary (Dr. Drummond), he was gratified that they were in harmony with his own. That gentleman had pointedly referred to three powerful influences in the transmission of pulmonary phthisis, viz. : the hereditary influence, unsanitary surroundings, and contagion, and happily they were in accord with his own. He also concurred with Dr. Drummond as to the importance of maintaining the health of those who were hereditarily predisposed to the condition. For he believed that should such an individual, while in the enjoyment of health, be exposed to the influence of the bacillus, no ill effects would ensue ; but on the contrary, if the health was impaired when such exposure had occurred, either through the immediate partner or in any other way, it was highly probable that sooner or later ill

would result. In answer to a further question from the President respecting the possibility of the bacillus of tubercle being dormant for a time, he stated that it was well known that the poison of enteric fever and cholera asiatica might lie dormant and inoperative for a varying interval of time, and he took the same view respecting the bacillus of tubercle. Dr. Philipson concluded by thanking the members for the cordial manner in which they had received his remarks.



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